ANCIENT EGYPT AND THE HEBREW MONARCHEIES
A REVIEW ARTICLE
K.A. Kitchen

Dr. Kitchen is Personal and Brunner Professor emeritus of Egyptology at the University of Liverpool, with long and wide experience in Egyptology, Semitics, and the civilisations of the biblical world, and a prolific author (including standard works of international status) in all these fields.

David, Solomon and Egypt, A Reassessment, JSOTSup 297

Paul S. Ash
Sheffield: Sheffield Academic Press, 1999,
157 pp., h/b., £37

Israel und Ägypten in der Königszeit. Die kulturellen Kontakte von Salomo bis zum Fall Jerusalems, OBO 170

Barnd Ulrich Schipper

Sennacherib’s Campaign to Judah. New Studies, SHCANE XVIII

William R. Gallagher,

In Palestine, during the 11th to 6th centuries BC, the Hebrews passed from being a group of tribes under local leaders (‘judges’) into a multi-tribe group under one leader as king (Saul), and then a fully-fledged monarchy that gained effective dominance in Canaan and further north (David, Solomon) in the 10th century BC. It then broke down (c. 930 BC) into two lesser kingdoms, one (Israel) swept away by Assyria c. 722 BC, and the other (Judah) ended by Babylon in 586 BC. In Egypt, this span of time corresponds to the Third Intermediate Period (21st to 25th Dynasties, c. 1070–664 BC) and the first 80 years of the 26th (Soite) Dynasty (664–525 BC). During this epoch, OT books (notably Kings, Chronicles, Isaiah, Jeremiah) occasionally mention contacts with Egypt, while a limited number of Egyptian sources, supplemented by Assyrian records, reflect contacts with Palestine that sometimes link up with the biblical narratives.

From the outset it should be stressed that in this period the Egyptian sources are very limited indeed, because (i) Egypt’s kings ruled from Memphis and the Delta, where almost all historical

records have long since perished, and (ii) most inscriptions from the better-preserved southern sites (e.g. Thebes) have no bearing at all on foreign affairs, but are mainly ritual or funerary. Thus the non-mention of biblical people and places in Egyptian texts c. 1100–580 BC merely reflects the poverty of our Egyptian documentation; it does not imply the non-existence or non-being of biblical people, places or episodes, as is sometimes alleged on the current fashionable wave of ultra-scepticism or ‘minimalism’ concerning the date and contents of the Hebrew Bible.

Three recently published books on this period and topic concern us here. Two illustrate the minimalist approach (Ash, on David and Solomon only; and Schipper, on the whole monarchy), while a third attempts a more even-handed review of its theme (Gallagher, period around Sennacherib’s Palestine campaign 701 BC). For the sake of simplicity, clarity and concision, it is best here to tackle the subject by successive themes.

Chronological Setting

Neither Ash nor Schipper has any expertise in this topic, and it shows. Ash claims (26) that the death of Solomon and accession of Rehoboam (in whose 5th year Shishak invaded) cannot be dated any more precisely than within about 50 years, c. 979–922 BC, and that it is ‘impossible’ (34) to dead-reckon Egyptian dates back from 664 BC, the agreed date for the start of the 26th Dynasty. Both claims are entirely false. In fact, we can validly reckon back from 664 BC. Before that date, Taharqa definitely reigned during 690–664 BC: 12 years’ minimum is needed for Shebitku (702–690) who had troops brought up from Nubia in 702/1 BC to oppose Sennacherib; before him, Shabako reigned 14 years, of which 13 in Egypt (716/5–702); and the 24th Dynasty king Bakenrafe reigned in 720–715. Before this, the reign-lengths of the attested 10 kings of the 22nd Dynasty back to the accession of Shoshenq I (Shishak) go back within narrow limits to 945/942 BC, 945 being preferable. This gives maximum range of up to 3 years, not 50! As in Egypt, so in Palestine. Ahab of Israel lived till 853, since he was involved in the Battle of Qarqar in 853, but Jehu ruled already by 841 BC, since he submitted then to Shalmaneser III. The two intervening Israelite kings fit exactly between these dates, on non-accession-year dating (as picked up by Jeroboam I from Egypt). On the biblical figures for

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Egypt. Already c. 2400 BC, starving foreigners sought help from king Unis; about 500 years later, kings Merikare and Amenemhat I had to oppose Canaanite incursions into Egypt; c. 1206 BC under Merenptah, the Edomites came to the East-Delta to pasture and water their livestock. (iii) Hadad’s pharaoh and royal mother-in-law are not named, hence were fictional. Wrong: Ramesses II, for example, married two successive Hittite princesses, but in the relevant inscriptions neither their parents (the Hittite emperor and queen) nor the princesses are named, except for giving a new Egyptian name to the first one. But, there is no fiction here; so, neither is there with Hadad. (iv) An Egyptian princess would not marry a foreigner. Wrong: see below under Siamun. By contrast, Egyptian elements in the narrative include the apportionment of houses, food supply and land (as source of income) for Hadad, and the term Tahpenes, which is either a transcript of the Egyptian word(s) for ‘queen’, or a similar-sounding Egyptian name.4

Solomon’s Pharaoh and his Daughter

Again, both Ash (37–46; 112–19) and Schipper (24–28; 84–157) seek to deny the probability of relevant Egyptian evidence, and hence the historicity of the Egyptian campaign and royal marriage (cf. I Kgs 9:16, cf. 3:1). Solomon’s reign can be set at c. 970–930 BC; his marriage to a pharaoh’s daughter happened early in his reign, probably in the first four years (c. 970–966).4 In Egypt, before Shishak and Psusennes II, there reigned Siamun (c. 979/8–960/59 BC), who overlapped the first decade of Solomoni’s reign. Thus he is most likely to have been the conqueror of Gezer and Solomon’s in-law. From Tanis (biblical Zaanan), Egypt’s East-Delta capital c. 1070–715 BC, we have a damaged triumphal scene of Siamun in traditional pose smiting a foe, from a now destroyed temple wall.4 In a monumental context such scenes belong almost always to kings who fought real wars; but not when they appear merely as decoration on state barges or minor works of art. This distinction rules out the attempts by Ash and Schipper to deny the probable historical worth of this scene. On this relief, the fragmentary foe grasps a remarkable axe, with crescent-shaped double blades. Despite attempts to dismiss it as a shield (Green, Lance; Schipper, 27), a halter or even

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3 Significantly, neither Ash nor Schipper pay attention to the indispensable (and formidable) works on the chronology of the Hebrew monarchies by Thiele and Galli, which do not suit their minimalist agendas. These are:  

4 For references see TIP, 273–74 nn. 182–83; plus (briefly on Tahpenes) Y. Muchnik, Egyptian Proper Names and Loanwords in North-West Semitic (Atlanta: SBL, 1999). 228f.

4 She was in Jerusalem when the Temple and other works were in progress (1 Kgs 3:1.1), and Hiram’s timbers for the Temple came via Joppa, hence necessarily through Gezer, the town given to Solomon by his Egyptian father-in-law.

4 Weinstein’s description of Siamun’s scene as belonging to ‘an unidentified king, possibly a howler which he now disavows: he is equally wrong to think that the axe is anachronistic in any way. J. Weinstein, ‘Egyptian Relations with the East Mediterranean World’, in S. Gitin et al. (eds), Mediterranean Peoples in Transition in honor of Professor Trude Dothan (Jerusalem: Israel Exploration Society, 1998), 1921. and n. 10; his doubts about historicity are also unfounded.
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handcuffs (Ash. 45). It is beyond doubt an axe.° Despite Schipper (261), Siamun’s example is wholly different in shape from the modest Palestinian-type double axes (as his figures 2 and 3 show clearly, p. 296). There is no true parallel as yet for Siamun’s axe-head from anywhere in the Near East; the nearest parallels are from the Mycenaean world of the Aegean and the Balkans, but are not precise. The uniqueness of Siamun’s example suggests a historical original still to be unearthed, and the reflex of a historical event. Immediately over the border of Egypt and North Syria, the Philistines are the first group that Siamun would encounter (from Gaza onward). That be and Solomon combined to crush the Philistines’ (economic?) power is a relatively safe working hypothesis, but one that makes sense. Sanaalite Gezer was conveniently reduced at the same juncture; so Solomon gained access to this strategic site, while Siamun probably laid tribute on the Philistine pentapolis, and both rulers could dictate trade-conditions (e.g. end of transit tolls?) to the Philistines. Thus there is no factual ground whatsoever to dismiss the historicity of either Siamun’s scene or of the biblical reports.

The same goes for the royal marriage. OT scholars tell us ad nauseam that Egyptian pharaohs never ever gave away their daughters away to foreigners (or commoners), always citing the remarks of Amenophis III (c. 1380 BC) to this effect. But what was true in the 14th century BC was not necessarily binding almost half a millennium later, in Solomon’s time. The kings of the 21st and 22nd Dynasties did give their daughters to foreigners and commoners, as I have long since documented.9 The Libyan lineage of Shishak (Shoshenq I) was explicitly treated as foreign, e.g. the Thebans referred to him not as king but merely as ‘the Great Chief of the Mashwash = Libyans’, and marked his name with the ‘throw-stick’ sign for foreigner. Thus when Psusennes II (last king of the 21st Dynasty)4 married his daughter Maatkare to Osorkon, Shoshenq’s son and heir (before Shoshenq’s accession, never mind Osorkon’s, courta Ash. 117), he was indeed marrying off his daughter to a foreigner. All this completely contradicts Ash’s denial (118) that I

have no evidence for non-Egyptian marriages for Egyptian princesses. This is no different to Siamun marrying off a daughter to Solomon, or a predecessor marrying off a kinswoman to Hadad of Edom. Examples with commoners are numerous, and are in total contrast to New-Kingdom practice of 500 years earlier.10 So criticisms of the Egyptian political marriages with Hadad and Solomon are misconceived. Non-mention of proper names of the pharaoh or his daughter are frivolous reasons for disbelief: the Amarna letters (14th century BC) do not name most royal daughters who pass to the Egyptian court, nor does Rameses II in documents on his Hittite marriages. By contrast, Solomon’s concern to build a house for pharaoh’s daughter (1 Kgs 7:5-9:24) mirrors precisely a royal concern to provide a fitting dwelling for the exotic newly-wed, exactly as Rameses II had done for his first Hittite bride, according to both Egyptian and cuneiform sources (an opposite comparison overlooked by Ash and Schipper, but not by real experts like Edel).11 The dowry for Solomon at Gezer was not simply a smoking ruin (Ash, 118), but the entire city-state with its strategic position and surrounding terrain. Thus, in contrast to Ash and Schipper (among others), there is no valid reason for doubting the veracity of these narratives. No evidence against them exists, and some indications point modestly in their favour.

Solomonic Trade

Ash (119-22) dismisses Solomon’s trade with Egypt and Que (= Cilicia in South Turkey) as royal propaganda. But trade is not an ancient near-eastern propaganda theme. Schipper (73-83) follows similar lines. They insist that horses were not bred in Egypt, and that no evidence exists for the horse/chariot trade. The former point is simply not proven, the latter is misuse of the lack of source-references. Horses were not native to Egypt, but first came there in the 12th Dynasty and Hyksos period (17th century BC), and in quantity during the New Kingdom. They were bred in Anatolia, and were traded south via Que (Cilicia). But Egypt could not import from such an area when it was at war with the Hittites who ruled these lands, so it almost certainly did breed horses for military purposes then. There was very extensive stabling and a ‘horse stud’ at Pi-Ramesse, for example.12 The Egyptians certainly manufactured chariots extensively in the New Kingdom; we have scenes of this in

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7 The axe should be studied from the original photo (Monteil, Osorkon II, 1947, pl. IX), not from the very poor line drawing that is regularly reproduced. The photo shows clearly part of a handle coming down from the socket of the axe-head. The axe grasps its thus, so that he cannot harm Pharaoh with it. Similarly, the foes in Shishak’s great scene hold their daggers by the (sheathed?) blade, not the handle, for the same reason. Ash’s remarks about Egyptian triumph-scenes (410ff.) are in error. There is too much to list here: certainly, Siamun’s does not violate... the genre (431). The axe is not ‘handcuffs’: in Egypt, these were an ornamental device with a central slot; see K. Lange, M. Hirmer, Egypt. Architecture. Sculpture, Painting (London: Phaidon Press, 3rd ed., 1961), plate 200. 8 See my TIP, Table 12, pp. 479 plus 594. 9 Despite recent statements to the contrary, the 21st Dynasty was not itself Libyan. All its kings were Egyptian in name and in lineage (where known), with only one exception, Osorkon the Elder (‘Oschor’), who reigned briefly before Siamun. The parallel Theban line of military governors was essentially Egyptian (Phankh onwards), with some Libyan links which are not yet precisely defined (secondary wives?).

10 This gives the lie to the false distinctions made by Schipper (87ff., where his mistreatment of my work ignores my 1986 and 1996 editions). Ash, Syp and others.


12 Cf. briefly E. Pusch, Egyptian Archaeology 14 (1999), 13 (area QIV).
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toms, and the large chariot-installations at Pi-Ramesses. There is thus no reason to deny continuing Egyptian upkeep of war-horses and chariot-building in later times, as armies still used them later. Thus, chariots could very easily have been bought from Egypt with some horses, and horses from Que as 1 Kings 10:28-29 states. Ash offers data on the export of horses from Egypt, as also to Alasia (with gold chariot), to the Hittite king Hattusili III, and to Sargon II. His minimalistic scepticism fails to take into account the paucity of sources on most topics in the 10th century BC. Ikeda’s notes on prices are unmisleadingly (121, n. 66). The price of horses sank steadily from the second millennium BC into the first; and varied by age, sex and quality of animals, then as now. Thus there is no exaggeration or fantasy here. Solomon traded with neighbouring royal courts (Hittites, Arameans), hence these were special steeds and ‘Rolls-Royce’ chariots in terms of cost. Schipper’s inadequate notes on other trade (e.g. Red Sea) should be supplemented or replaced by other recent studies.15

One positive contribution is Ash’s survey (64-97) of Egyptian-type artifacts found in Canaan from periods Iron IB/IIA, covering the United Monarchy period. Likewise, Schipper has a similar survey (41-56), with similar findings. Not surprisingly, most of this bric-a-brac comes from the coastal and adjoining regions, owing to much trade passing by sea and seaports. Almost none of these bits comes from the highlands (Judah/Israel), so they conclude that there was no Egyptian trade with Solomon’s kingdom. But this logic is flawed. (i) These paganising scarabs and amulets would have no attraction (or practical value) for the largely Yahwistic peasantry of Judah and Israel. (ii) They cannot tell us anything about the volumes of exchangeable perishable goods which left no trace. (iii) Elite trade was exclusively with capitals and royal courts, not with highland farmsteads; with the wreck of pre-exilic Jerusalem, all that has disappeared. One may as well try to produce a total picture of modern world trade by counting the number and variety of cigarette-cartons on our beaches today!

The Campaign of Shishak (Shoshenq I) in Palestine

For this event, we have five possible witnesses from outside the OT: the great scene and list of place-names in Judah, Israel and Jordan at Karnak; a mere fragment of a similar scene at El-Hibeh; part of a stela of Shoshenq I from Megiddo; an allusion of the coffin of one of Shoshenq’s followers; and a broken stela at Karnak about a border-incident. The last-named incident might well have provided Shoshenq I with his excuse for raiding post-Solomonic Canaan, while his ultimate aim was much more. El-Hibeh and the coffin contribute but little. As for the great Karnak list, Ash gives a tolerable account of its contents and the king’s campaign, rightly surmising that Shoshenq aimed at domination of Canaan (50-56). But errors abound. (i) Ash still insists that ‘Jerusalem should appear’ in it (54). But the city submitted to Shishak cf. 2 Chr. 12). It was not captured by storm, and so may not even have been listed. Also several names are wholly destroyed in Row IV of the List, and Jerusalem might have been any one of these. (ii) The superscription to the list is not stereotypical prose (52, based on BreastEd’s out-dated translation). Rather it is in regular parallelistic poetry, it refashions traditional concepts and language, and it introduces novel features.16 (iii) Ash does not properly understand my reading of name 105/106 as ‘Highland of David’ (54, n. 166). He cannot read hieroglyphs correctly: the w- sign is not a chick(!) but the curved rope (u3), and is definitely consonantal in names 76, 91 because it is initial; it is not ‘problematic’ in any way. And if final d can be rendered by voiceless t in Ethiopic, where Davut is definitely ‘David’, then it is possible in Egyptian, which is also Afro-asitic. Bayt-Dawut is not just a place-name, but a personal dynastic term for Judah, as is Assyrian Bayt-Omri for Israel. These terms do imply a personal David and Omri as dynastic founders.

Schipper too (119-20) gives a fair summary of Shoshenq’s campaign from the list, but errs in querying the year of Rehoboam (see §1, Chronology, above). His ‘critical assessment’ of the OT text is largely misguided (122-25). He accuses the list of numerous repetitions, especially of the word ngrb, ‘Negev’, but does not realise that these occurrences are only half a name, each time to be combined with the following name-ring (‘the Negev of X, the Negev of Y, etc.’), as with the terms hgr, ‘enclosure’ (recognised in 129 n. 81) and hdb’t, ‘highland’. Of the ‘highland of David’, he seems blissfully unaware.17

Alongside the great Karnak List, we have the Megiddo stela, which proves that Shoshenq I did reach and take over that town, almost certainly during this campaign.18 Astonishingly, this stela is barely

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17 For a detailed treatment of relations with Ophir, Arabia, etc., see my chapters ‘Egypt and East Africa’ and ‘Sheba and Arabia’, in H. K. Handy (ed.), The Age of Solomon, Scholarship at the Turn of the Millennium (Leiden: Brill, 1999), 106-25 and 126-53.
tombs, and the large chariot-installations at Pt-Ramesse.19 There is thus no reason to deny continuing Egyptian upkeep of war-horses and chariot-building in later times, as armies still used them later. Thus, chariots could very easily have been bought from Egypt with some horses, and horses from Qe as 1 Kings 10:28-29 states. Ash omits data on the export of horses from Egypt,20 as also to Alasia (with gold chariot), to the Hittite king Hattusili III, and to Sargon II. His minimalistic scepticism fails to take into account the paucity of sources on most topics in the 10th century BC. Ikeda’s notes on prices are used misleadingly (121, n. 66). The price of horses sank steadily from the second millennium BC into the first; and varied by age, sex and quality of animals, then as now. Thus there is no exaggeration or fantasy here. Solomon traded with neighbouring royal courts (Hittites, Arameans), hence these were special steeds and ‘Rolls-Royce’ chariots in terms of cost. Schipper’s inadequate notes on other trade (e.g. Red Sea) should be supplemented or replaced by other recent studies.15

One positive contribution is Ash’s survey (64-97) of Egyptian-type artifacts found in Canaan from periods Iron IB/IIA, covering the United Monarchy period. Likewise, Schipper has a similar survey (41-56), with similar findings. Not surprisingly, most of this bric-a-brac comes from the coastal and adjoining regions, owing to much trade passing by sea and seaports. Almost none of these bits comes from the highlands (Judah/Israel), so they conclude that there was no Egyptian trade with Solomon’s kingdom. But this logic is flawed. (i) These paganising scarabs and amulets would have no attraction (or practical value) for the largely Yahwistic peasantry of Judah and Israel. (ii) They cannot tell us anything about the volumes of exchangeable goods which left no trace. (iii) Elite trade was exclusively with capitals and royal courts, not with highland farmsteads; with the wreck of pre-exilic Jerusalem, all that has disappeared. One may as well try to produce a total picture of modern world trade by counting the number and variety of cigarette-cartons on our beaches today!

The Campaign of Shishak (Shoshenq I) in Palestine

For this event, we have five possible witnesses from outside the OT: the great scene and list of place-names in Judah, Israel and Jordan at Karnak; a mere fragment of a similar scene at El-Hibe; part of a stela of Shoshenq I from Megiddo; an allusion of the coffin of one of Shoshenq’s followers; and a broken stela at Karnak about a border-incident. The last-named incident might well have provided Shoshenq I with his excuse for raiding post-Solomonic Canaan, while his ultimate aim was much more. El-Hibe and the coffin contribute but little. As for the great Karnak list, Ash gives a tolerable account of its contents and the king’s campaign, rightly surmising that Shoshenq aimed at domination of Canaan (50-56). But errors abound. (i) Ash still insists that ‘Jerusalem should appear’ in it (54). But the city submitted to Shishak (cf. 2 Chr. 12). It was not captured by storm, and so may not even have been listed. Also several names are wholly destroyed in Row IV of the List, and Jerusalem might have been any one of these. (ii) The superscription to the list is not stereotypical prose (52, based on Breasted’s out-dated translation). Rather it is in regular parallelistic poetry, it refashions traditional concepts and language, and it introduces novel features.16 (iii) Ash does not properly understand my reading of name 105/106 as ‘highland of David’ (54, n. 166). He cannot read hieroglyphs correctly: the w- sign is not a chick(!) but the curved ope (u3), and is definitely consonantal in names 76, 91 because it is initial; it is not ‘problematic’ in any way. (iv) If final d can be rendered by voiceless t in Ethiopic, where Dawit is definitely ‘David’, then it is possible in Egyptian, which is also Afro-asiatic. Bayt-Dawit is not just a place-name, but a personal dynastic term for Judah, as Assyrian Bayt-Onni for Israel. These terms do imply a personal David and Omri as dynastic founders.

Schipper too (119-20) gives a fair summary of Shoshenq’s campaign from the list, but errs in querying the year 5 of Rehoboam (see §1, Chronology, above). His ‘critical assessment’ of the OT text is largely misguided (122-25). He accuses the list of numerous repetitions, especially of the word nrb, ‘Negev’, but does not realise that these occurances are only half a name, each time to be combined with the following name-ring (‘the Negev of X, the Negev of Y, etc.), as with the terms hgr, ‘enclosure’ (recognised in 129 n. 81) and hdyrb, ‘highland’. Of the ‘highland of David’, he seems blissfully unaware.17

Alongside the great Karnak List, we have the Megiddo stela, which proves that Shoshenq I did reach and take over that town, almost certainly during this campaign.18 Astonishingly, this stela is barely

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19 See the new and complete translation in Kitchen, Poetry of Ancient Egypt (Jonsérur: Paul Aströms förlag, 1999), 433-40, with introduction and notes.
20 On the probable mention of ‘the highland of David’ at Nos. 105/106 of the Shoshenq list, less than 50 years after David’s death, and 100 years before the Tel Dan stela, see Kitchen, ‘A Possible Mention of David in the Late Tenth Century BCE, and deity *Dad as dead as the Dodo?’, JSOT 76 (1997), 29-44; popular summarisation by H. Shanks, ‘Has David Been Found in Egypt?’, BAR 25/1 (1999), 34-35.
21 Note in passing that the paper by T. Clancy, ‘Shishak/Shoshenq’s Travels’, JSOT 86 (1999), 3-23, is wholly unreliable. He wrongly tries to reattribute the Megiddo stela of Shoshenq I to a king Shoshenq IV (whose second cartouche is entirely different), and to down-date Shoshenq I to c. 800 BC following the maverick David Rohl. His views of Jewish priests penetrating Karnak Temple to find the Shoshenq scene are pure fantasy, as is most of his geographical ‘rewriting’ of Shoshenq’s campaign.
mentioned by Ash (56), though it is rightly seen to indicate that Shoshenq intended to maintain control over Canaan, a plan ended by his sudden death. With the same basically correct interpretation of the stela, Schipper (129–32) is more thorough, but he too exhibits a basic inability to read hieroglyphs. Shoshenq’s prenomen here (as everywhere else) Hedj/khepereru Setepenamun, not Hedj/khepereru Setepenamun (contra Schipper, 130f.), and the damaged group over the Shoshenq-cartouche cannot possibly be ‘Son of Re’ (goose + circle), but is ‘Lord performing the rites’ (Neb ir khet).

Such is the very inconsistent quality of these two books. This brings us to the end of Ash’s work, except to say that his end-note on Sheba at least attributes its queen to the right part of the map (Arabia). He has an extensive bibliography and brief indexes.

From Osorkon I to Osorkon II

For the period of the twin Hebrew monarchies, we are concerned solely with Schipper until 701 BC. Osorkon I (c. 924–889 BC), son and successor of Shoshenq I/Shishak, concerns us on three counts. First, he, his father Shishak and his grandson Osorkon II all maintained relations with Byblos, and broken statues of each of these kings were found there. To those of Shoshenq I and Osorkon I, the local kings of Byblos (Abibael and Elibaal) added dedications to Phoenician to their local goddess, Baalat-Gebal, ‘the Lady of Byblos’. Most commentators correctly regard these statues as marking the good relations between Egypt’s rulers and Byblos, via which they obtained Phoenician timber. The pharaohs dedicated them in the temple of Baalat-Gebal, where the local kings then added their own dedications. Other views exist, e.g. that of Redford, that these pharaohs imposed their rule on Byblos, for which there is no justification. And now, at the opposite extreme, Schipper dismisses them as being pieces traded to Byblos and only later reused by the Phoenician kings. Again, for such a view there is no justification. On the contrary, the statues show that Egypt’s involvement with the Levant did not cease at the death of Shishak.

Secondly, just after his father’s death, in years 1–4 of his own reign, Osorkon I embarked upon a spending-spree of unparalleled dimensions. He gave nearly 400 tons of silver and gold to the temples of Egypt, much of this being listed in detail. Some may have come from trade, and some from inherited wealth of the pharaonic state. But it is surely more than coincidence that this spending-spurge (like his father’s, on huge new temple-buildings in Thebes and Memphis) immediately followed the looting of the gold, etc., from Solomon’s and Rehoboam’s Jerusalem.

Thirdly, there is the mysterious notice in 2 Chronicles 14:9–15 about Zerah the Kushite who briefly invaded Judah with a myriad of troops and 300 chariots, to be defeated by Asa in his 14th year, c. 919 BC which is contemporary with Osorkon I. In line with nineteenth-century dogma, Schipper dismisses the Chronicler here as inventing the incident for theological reasons (133–39). This will not do: the Chronicler often preserves authentic material not otherwise available to us. A very good example is the Sukkiiim in Shishak’s forces (2 Chr. 12:3), not mentioned in Kings, but known to be the Libyan Tjukten of Egyptian texts (a fact ignored by Schipper). Rush is Nubia, along the Nile south of Egypt. No army of Kush could reach South Palestine except via the Red Sea (impractical and unknown), or via Egypt (impossible unless with a pharaoh’s backing and control). Kush(an) was not a current term for Edom since patriarchal times, and 9th-century Edom would not have a vast force with 300 chariots. It is for this reason that the perfectly sensible suggestion has often been made, that it was Osorkon I who sought plunder in Judah (aping his father’s exploit), but was content simply to send a large force under a general instead of going himself; and this failed. Defeat is never celebrated by any pharaoh officially, so we can expect no word of it from Osorkon I.

Two reigns later, we come to Osorkon II (c. 874–850 BC; not 874–835/30 BC, as Schipper following Aston). By 853 BC, Assyria had become a menace to the lesser kingdoms of Syria and Palestine, so in 853 BC many of them jointly resisted Assyria at the Battle of Qarqar. Among these allies, came ‘1,000 soldiers from Musri’, i.e. Egypt. This identification for Musri is clear and beyond doubt: the only other Musri was east of the Tigris and not involved in Syria. There was no other, as the Arabian and North-Syrian ‘Musris’ are spurious, despite Schipper’s attempt to revive the Syrian one (144–49). The ‘Musri’ of the Black Obelisk of Shalmaneser III sent ‘tribute’ that included hippos, rhinos, and monkeys, indubitable African fauna. That Osorkon II allied himself with the Levant kings against Assyria is supported by the finding of an alabaster presentation-vessel of Osorkon II in the ruins of the palace of Omri and Ahab at Samaria, the kind of prestige object that pharaohs commonly sent in such cases. Schipper typically dismisses the piece as stemming from Phoenician trade (177–81). But ordinary trade was with fluids in pottery amphorae, not alabasters. Alabasters were for very precious ointments and the like; the assumption of Phoenician...
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18 A view derived from the study by D. Ussishkin, ‘Notes on Megiddo, Gezer, Ashdod and Tel Batash in the Tenth to Ninth Centuries B.C.’, BASOR 277/278 (1990), 71–91, esp. 71–73.
19 For more (and pertinent) information and analyses see my essays in Handly n. 15 above.
20 Schipper (177–77) cites Helck’s view that Middle-Kingdom statues were thus removed from Egypt to Syria under the Hyksos: but 500 years earlier, the situation was radically different, and Helck’s view is not absolutely proven in all cases for the Middle Kingdom either.
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So and Osorkon IV

Much ink has been spilt over ‘So, King of Egypt’ in 2 Kings 17:4. The nearest king of Egypt to whom Hoshea might send was the 22nd Dynasty in Tanis (Zoan). In 725 BC, date of that event, the king there was certainly Osorkon IV (premenen, Akheperre), attested in Bubastis by the Nubian conqueror Pharaohkhan in 728 BC. His 23rd-Dynasty neighbour was king Iuput II, further south-west at Leontopolis, not known to be visited by any Hebrew envoy. Tefnakht I, Ruler of the West, reigned at Sais deep in the swamplands of the West-Delta. Contra some OT scholars, he played no part whatsoever in Near-Eastern politics, nor did his city Sais (sometimes illegitimately emended into the Hebrew text to replace ‘So’). All of this is accepted by Schipper (149–58), which is all to the good, given the factual basis for this situation, fully set out elsewhere. Osorkon IV did not attempt to aid the distant Hoshea in 725 BC, but did send his general Re’ to help Hanun of nearby Gaza (unsuccessfully) in 720 BC, and found it expedient to send a tributary gift of horses to Sargon II on his doorstep (at El-Arish), to buy him off, being named by the Assyrian ruler as Ushilkanni. Soon afterwards, Osorkon IV disappeared, with the Nubian (Kushite) reconquest of Egypt by Shabako in c. 715 BC.

The Years around 701 BC

Here, Gallagher’s recent work joins with Schipper. Schipper (210–16) is unduly influenced in his estimate of the value of the OT record (2 Kgs 18–19; Isaiah 36–39) by nineteenth-century criticism which automatically condemned as unhistorical any passage that invokes divine intervention. By contrast, rationalist scholars who examine ancient annals (e.g. of the Assyrians) find that these too are permeated with divine interventions (by the god Assur, etc.), yet habitually accept their historicity without demur! The same rule should apply to the OT and its neighbours alike.

Gallagher’s work on 701 BC begins by clearing the ground of preliminary matters. First, he surveys the sources for Sennacherib’s campaign, quite rightly dismissing the theory of two Palestinian campaigns of Sennacherib, for which there is not a scrap of respectable evidence. The only real reason for this theory is the occurrence of ‘Tirhakah, king of Kush’ in 2 Kings 19:9 (parallel Is. 37:9; strictly it is just the words melek Kush, ‘king of Kush’, since

in 701 Shebitku, not Taharqa, was king in the Nile Valley). It is now universally accepted that Taharqa did not rule supreme in Egypt and Nubia until 690. For many generations OT scholars assumed an anachronism without investigating matters further. But the answer lies in the biblical text itself: these very passages continue their story down to the death of Sennacherib (2 Kgs 19:36–37; Is. 37:38–39), which only happened in 681 BC. By then, Taharqa had already been king for 10 years, so the Hebrew narrators simply used his later title to identify him. Just as we would say today, ‘Queen Elizabeth II was born in 1926’, which she was, but not as queen then. It really is just as simple as that. On his stelae Taharqa himself uses the same technique of back-reference. Second, Gallagher usefully works through several passages in Isaiah (21; 22; 10:5–19; 14:4–21), showing that they refer to the situation in 705/4 and 704/3, not 701.

In chapters 3–5 Gallagher gives a lucid and convincing account of the Assyrian advance through Phoenicia to 701, and of Sennacherib’s bringing Philistia to heel. The heart of his book, chapters 6–9, is devoted to the next phase of Sennacherib’s enterprise, the reconquest of Judah, to which is added an Appendix on why Hezekiah rebelled. Gallagher’s examination of the Assyrian narratives is very illuminating, illustrating how their order is not always chronological but sometimes thematic. In chapter 7 he closely examines the Hebrew text of 2 Kings 18–19, considering 18:13–16 to be an overall summary (often called ‘source A’) separate from 18:17–19:37 (‘source B’). This may be so, but is by no means proven. On the other hand, he is well able to show that there is no factual basis for arbitrarily dividing up ‘B’ into two intertwined ‘sources’ B1 and B2; this can be dismissed. Chapter 8 returns to historical evaluation, and brings to bear a mass of valuable material on many points, such as the use of propaganda by the Assyrians (using data from both Assyria and World War II to illustrate the techniques used). From his treatment overall, the battle at Eltekeh should not be confused with the subsequent threat of a further attack by Egypto-Nubian forces [with Taharqa], while the Assyrians were occupied with Jerusalem and Lachish and Libnah. The speeches of the Rab-shaqeh to the Jerusalemites find good background for authenticity. And there is much else besides. The book closes with a useful select bibliography and good indexes.

The 7th and 6th Centuries BC

Here, we return to Schipper to complete the epoch. During 674–664 BC, the intrigues and interventions by the Kushite kings of Egypt with the Levantine kinglets drew down the wrath of Assyria, so that Esarhaddon and Assurbanipal repeatedly invaded Egypt to subdue her, imposing Assyrian garrisons in the Delta to ward off

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Psamtek I quietly reunited Egypt and kept alliance with Assyria
while rejecting her overlordship. By then, Assyria could not argue,
being pressurised from the east. This is all treated reasonably well
by Schipper (217ff.), along with trade-contacts on the limited finds
known (247ff.), Egyptians in Mesopotamia (265ff.) and in Hebrew
sources (272ff.), and Levantines in Egypt (277ff.) Most of this is
useful and informative. The book ends with a final summary,
extensive bibliography and moderate indexes.

In Closing
To sum up, Ash’s work is the poorest of the three: its minimalist
stance is not justified by the known facts and background, the work
is frankly superficial, despite an outward show of erudition.
Schipper’s industrious work is far more thorough in its more
extended field, and contains much of value, but is also marred by its
unrealistic nihilism and its nineteenth-century style of subjective
criticism, which cannot be justified on the known external facts, the
only evidence that counts. Gallagher’s large work on one historical
episode is an altogether more even-handed and more objective
treatment of the biblical and other texts, and is of very considerable
value, his faux pas over Taharqa excepted. Use and enjoy Gallagher,
use Schipper very critically, forget Ash!

ARE THERE ESSENTIAL DIFFERENCES BETWEEN THE SEXES?
Sharon James
Sharon is a consultant editor of Themelios. She and her
husband Bill lead the Council on Biblical Manhood and
Womanhood in the UK. Her biography of Ann Judson,
My Heart in His Hands is published by Evangelical Press.

Created or Constructed? – The Great Gender Debate
Elaine Storkey

Elaine Storkey is regarded by many as the leading British evangelical
spokesperson on gender issues. Her latest book directly addresses the
vital question ‘created or constructed?’ This review article summarises
each chapter, and then offers a critique.

Created or Constructed? Three words summarise a debate that rages
through university campuses. During the 1960s and 1970s it
became popular to assert that all the masculine and feminine
characteristics which we associate with being male or female are
purely the result of social conditioning. They are not essential to our
being. In other words sex – the biological fact of being male or female
is a given. But gender – maleness and femaleness – is an artificial
social construct from which we need to be liberated. So Judith
Lorber writes:

When we no longer ask ‘boy or girl?’ in order to start gendering
an infant, when the information is as irrelevant as the colour of
a child’s eyes ... only then will men and women be socially
interchangeable and really equal. And when that happens there
will no longer be any need for gender at all.

From the 1980s onwards, sex itself has been viewed by some as a
construct. In universities it is commonplace to hear that sexuality is
plastic. The very idea that there is any essential difference between
the sexes has been defined by some academics as heresy.

1 Quoted in Germaine Greer. The Whole Woman. Doubleday. London,
1999. 324.

2 Robert S. McElvaine defines essentialism as the heresy that there are
biological differences between males and females. Wendy Shalit, A Return
to Modesty. Touchstone, 2000. 87. Shalit discovered this heresy in a
first year philosophy class. When she mentioned ‘difference’ between the
sexes she was denounced as an ‘essentialist’. ‘What’s that?’ she enquired.
‘Someone who believes in differences between the sexes’ replied her
classmates. ’But aren’t there?’ she asked. ’No!’ they all chorused.
She went on to write A Return to Modesty, which argues that many young
women today are pushed into promiscuity, and miss the beauty and
romance of preserving their virginity for their husband. Along the way
there are clear-sighted denunciations of those who deny the obvious
differences between men and women.